Chapter 3
The Development of Carnot's Mechanics

It is fortunate that the two memoirs Carnot submitted to the *Academy of sciences* in Paris in 1778 and 1780 have survived in its archives. They give access to the form and content of his thinking about mechanics and the science of machines when he was a young and hopeful officer 5 or 6 years out of engineering school. The present chapter traces the development of his thinking from these, its earliest recorded expressions, through to the publication, in (1803a), of his *Principes fondamentaux de l'équilibre et du mouvement*. The method is to compare these earlier and later versions to the *Essai sur les machines en général*, and to continue to an analysis of Sadi Carnot's *Réflexions sur la puissance motrice du feu* of 1824 in order to bring out its filiation with his father's work. The relevant, theoretical sections of the early two memoirs are reprinted by Charles Gillispie in his *Lazare Carnot Savant* (Gillispie 1971, Appendices B and C, pp 270–340). Readers may wish to refer points in the discussion, or indeed the discussion as a whole, to the original texts.

3.1 Argument of the 1778 Memoir on the Theory of Machines

A notation on the cover page of the manuscript of Carnot's first memoir indicates that he learned of the Academy's announcement of a prize contest from the *Gazette de France* of 18 April 1777. The Academy specified the subject to be:

> The theory of simple machines with regard to friction and the stiffness of cordage, but it [the Academy] requires that the laws of friction and the examination of the effects resulting from stiffness in cordage be determined by new experiments conducted on a large scale. It

C.C. Gillispie and R. Pisano, *Lazare and Sadi Carnot: A Scientific and Filial Relationship*, History of Mechanism and Machine Science 19, DOI 10.1007/978-94-017-8011-7_3,

> requires further that these experiments be applicable to machines used in the Navy such as the pulley, the capstan, and the inclined plane.[1]

From a further note it appears that the Academy received the entry Carnot sent on 28 March 1778. Assuming that he had set to work immediately, it took him, therefore, just under a year to design and carry out his experiments and compose the argument. For device he misquoted a line from Lucretius: "Videndum/Qua ratione fiant et qua vi quaeque gerantur"[2] (Gillispie 1971, p 271). In setting the competition the responsible members of the Academy would have had no thought that anything of importance to the science of mechanics itself was to be expected in consequence of the entries it might elicit or on the part of the contestants it might attract. The emphasis was on the naval application, and what the Academy clearly wanted was studies of friction. None of the papers having satisfied the judges, it reset the same subject for the contest of 1781, and then awarded the prize to Coulomb, whose investigation gave them exactly that and clearly deserved to win. His memoir *Théorie des machines simples en ayant égard au frottement de leurs parties et à la roideur du cordage* (Coulomb 1785, pp 161–332) remains one of the cardinal contributions to the knowledge of friction.[3]

---

[1]The translation is from original manuscript of which a photographic reproduction was edited in: Gillispie (1971), Appendix B, pp 270–296. Hereafter we quote Lazare Carnot's *Memoires* (1778, 1780) using «§» as reported in Gillispie (1971), Appendices.

[2]Probably relying on memory while working at Cherbourg, he had in mind line 129 of *de rerum natura*, Book I, but spoiled the scansion by inverting the word order; he also displaced the gerundive from the end of line 131. The entire passage reads

> Qua propter bene cum superis de rebus habenda
> nobis est ratio, solis lunaeque meatus
> qua fiant ratione, et qua vi quaeque gerantur
> in terris, tum cum primis ratione sagaci
> unde anima atque animi constet natura videndum,
> et quae res nobis vigilantibus obvia mentis
> terrificet morbo adfectis somnoque sepultis,
> cernere uti videamur eos audireque coram,
> morte obita quorum tellus amplectitur ossa.

A translation of the words Carnot chose might read: "Let us look to an account of how all things come to pass and of the forces that govern them".

[3]For a discussion of the contest centered upon the winning memoir, and for the importance of Coulomb's work on friction, see Gillmor (1971), chap. IV. The judges were d'Alembert, Bézout, Bossut, Condorcet, and Trudaine de Montigny.

Carnot's own taste and interest, however, responded primarily to the opening phrase, and he submitted his entry as a *Mémoire sur la théorie des machines*. It is divided into two parts, experimental and theoretical. As will appear, the organization of Part II already exhibited the structure within which Carnot's thought about the subject developed throughout his life. Obedient to the Academy's injunction, he did dutifully set to work to determine experimental laws of friction and binding, and Part I of the memoir consists of some 20 folio pages detailing the results. An account of these experiments would be no help in understanding the difference Carnot's work finally made in mechanics, but perhaps the reader will be curious to know what he undertook. Employing the services of an assistant, he spent considerable pains upon two sets of determinations, one concerned with friction and the other with cordage. For the work on friction Carnot constructed the device pictured in the following (see Fig. 3.1).

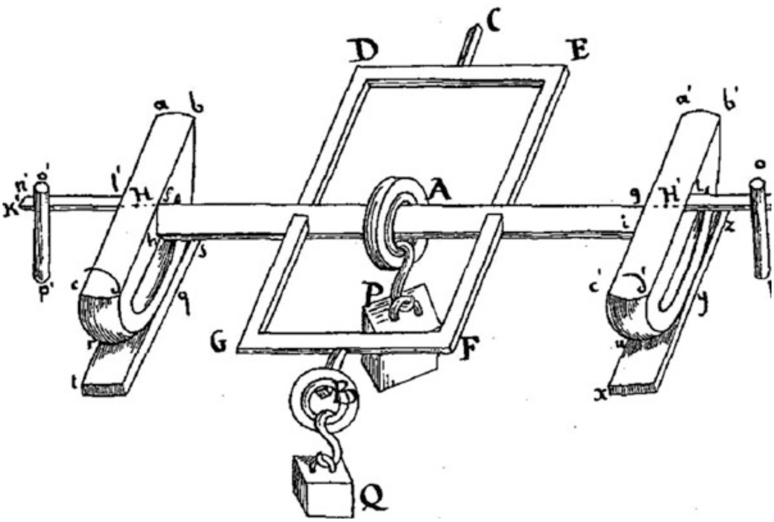

Fig. 3.1 Machine for valuing friction (The illustration is in fact from the 1780 memoir, but it is clear from the context that it is the same device of which the drawing has been separated from the 1778 text.)

The instrument operated in a simple manner for the purpose of determining values for friction between surfaces that (in the so-called first type) slide one across the other and (in the second type) roll one upon the other. The principle of the machine was that the statical moment of a weight Q (see Fig. 3.1) just sufficient to initiate rotation was the measure of the force of friction (with which it was in equilibrium) between the surfaces of the hemispheres and of the iron shoes *srtg* and *zuxy*. The inner circumference of the rings A and B and the upper blade of the axle HH' were knife edges.

With the retaining pins op and $o'p'$ in place, the axle was free only to turn. A sufficient weight Q would cause the surfaces of the hemispheres to slide across the supporting shoes, and the friction measured was of the first type. With the pins op and $o'p'$ removed, the axle was free to displace. A weight Q would cause the hemispheres to roll on the supports, and the friction measured was that of the second type. Carnot made, too, a determination of the effect of adherence. In measuring sliding friction, the values were always significantly greater if the device was allowed to rest on the supports for an appreciable time than they were when the measurement was made at the instant it was loaded. Adherence appeared to have no appreciable effect, however, upon rolling friction.

Carnot employed increasing weights for P and Q, and tabulated the ratios of the two classes of friction and of adherence to pressure over a range between 100 and 2,000 lb for the total weight of the movable cradle. In each class of measurement, the ratio of friction to pressure decreased with increasing load. In good engineering fashion, he sought to express the results in formulas that turned out no more beautifully than such empirical expressions usually do.

The formula for the ratio x of sliding friction to pressure was

$$x = \frac{A + Bp + Cp^2 + \ldots etc.}{1 + bp + cp^2 + \ldots etc.}$$

Carnot well understood its arbitrariness. He could have given the relation a simpler appearance, he observed, in supposing that

$$x = A + Bp + Cp^2 + \ldots etc.$$

except that as p increased, so would x, which effect was the contrary of what the experiments revealed. On the other hand, he would fall into the opposite error in supposing that

$$x = A + Bp^{-1} + Cp^{-2} + \ldots etc.$$

The best he could say for the formula adopted was that it fell as closely as possible between the two extremes.

Further experimentation to determine the influence of velocity on the two classes of friction led Carnot to produce formulas even more empirical in appearance. For sliding friction,

$$X = \frac{\phi + \mu\pi}{1 + \varpi\pi} \cdot \frac{1 + bu}{1 + cu},$$

and for rolling friction

$$X = \frac{\phi + \mu\pi}{1 + \varpi\pi} + \frac{\phi'}{1 + \varpi'\pi},$$

where x was the ratio of friction to pressure; ϕ, μ, ω, ϕ' and ω' were constants depending on the nature and degree of polish of the surfaces; π was the pressure exerted by a single point; u was velocity; and b and c were constants of velocity determined by a method at once cumbersome and inaccurate.

The experiments on stiffness and binding in cordage were of a similar type, and the results no more applicable. Perhaps it will suffice to reproduce the illustration of the pulley-type instrument that Carnot designed and employed in order to determine how the properties of ropes and wires affected the equilibrium conditions of weights of varying magnitude (Fig. 3.2).

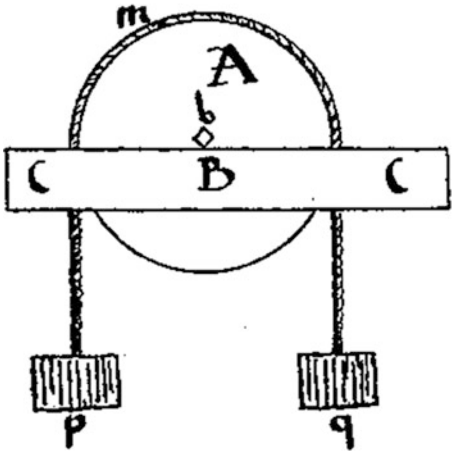

Fig. 3.2 Instrument for determining equilibrium conditions (Gillispie 1971, p 65)

The account of these experiments occupies § 1 through 26, some 16 folia of a manuscript containing 64 folia all told. At the outset Carnot promised that Part II would contain their "application to machines". What Part II actually contains is three sections, the first 16 folia discussing the principle of "machines en général" in § 27 through 50, and the remaining two, in § 51 through 84, applying the principles respectively to equilibrium and to motion in the cases of the seven types of simple machine. It is the first section of Part II that is most interesting to the historian, for it is one of those relatively rare documents that admits him to the genesis of an approach that was later to become distinctive of a whole new way of seeing and doing a science.

That it was an application of the experiments was a pretense on Carnot's part, however. It is difficult, in fact, to see that they had anything to do with the theoretical discussion of machines in general except to serve as an excuse to qualify the memoir for this particular competition. The excuse is the less convincing in that Carnot had not the time or did not take the trouble to fill up his tables of data completely. There is no mention of these experiments later in his published writings. Nor did there need to be despite his view of mechanics as an experimental science. The most that can be said, and as a highly likely conjecture this much ought to be said, is

that the instrument he constructed for measuring friction may very well have led him to set up problems in terms of torque and moments of rotation around an axis, and therefore towards the emphasis he placed upon the principle of moment–of–momentum in the *Essai sur les machines en général*.

However that may have been, the theoretical passages that were the heart of the memoir make clear how Carnot thought to approach the subject. He made no preliminary statements of basic principles of mechanics of any sort. Rather, he moved in imagination right into the interstices of hard body and pictured what the inflexible rods and inextensible wires were doing, mini–levers and mini–pulleys, micro–machines that transmit motion there where it starts, corpuscle to corpuscle. Once those interactions were clarified, they would be those ideally operating in the employment of any actual machines. That is what he thought about in the first instance: what one corpuscle was doing to another in a system of hard bodies. A motion was imparted to the system. The mutual interactions of the corpuscles transformed that motion into some other which it was the problem to determine. Designating by

m the mass of corpuscle,

V its virtual velocity (his expression was "[. . .] the velocity that it would have taken if it had been free, that is to say without the reaction it undergoes from the rest of the system [. . .]"[4]),

u the velocity it really did assume, and

y the angle between the directions of V and u,

then the following equation would hold:

$$\int mu\,(V\cos y - u) = 0,$$

which asserts that the sum of the products of the quantity of motion (mu) of each of the corpuscles multiplied by the velocity that it lost evaluated in the direction it took ($V\cos y - u$) is equal to zero (Gillispie 1971, Appendix B, § 29). For the formation of Carnot's idiom, the significant term was the one in parenthesis. It makes evident that from the very beginning he thought of interactions within a system in d'Alembert's terms of velocities lost or gained by its constituent parts, and thought of live force (which in later times would be seen as the energy involved in such interactions) as the product of momentum by a velocity or motion thus "lost" or "gained" (Gillispie 1971, Appendix B, § 27). The actual demonstration involved the physics of hard–body collision and equating the forces of action and reaction in pairs of contiguous molecules: in actual practice the equality of action and reaction was always the basic law of mechanics for Carnot.

In this memoir, Carnot claimed for the above equation the status of a fundamental theorem. It did duty for all the variants that he later elaborated in the *Essai sur les*

[4]Gillispie (1971), Appendix B, § 27.

machines en général–its determinate Equation (E) (see Eq. 2.1) for the general case, its transformation by way of geometric motions into the indeterminate Equation (F) (see Eq. 2.2), its deduction from Equation (F) of the principle of conservation of moment–of–the–quantity–of–motion with respect to any geometric motion, its derivation of the power Equation AA (see Eq. 2.3) concerning the forces applied to a system, its general principle of equilibrium and motion for machines in terms of the balance between moments–of–activity consumed and exerted by impelling and resisting forces. Carnot drew upon the general corpus of mechanics for all these refinements to his problem. In the early memoir only more or less definite foreshadowings of them occur scattered along a series of some 17 corollaries and remarks. Most were so elementary or trivial that one would never read them twice, and no emphasis was given to the ones that in the retrospect of Carnot's later writings seem significant, the kernels that later opened out into main positions. The most useful procedure will be to point out what elements he did then have of his later findings, and what the differences were.

The greatest difference in emphasis was that when writing of force in his early memoir Carnot began with the effects of accelerative or motive force in producing motions. In the *Essai sur les machines en général*, it will be recalled, accelerative force remained largely hidden from attention until near the end, when it appeared as one of the factors in moments–of–activity, i.e., as the force that does act across a distance or in a time. In the 1778 memoir, however, the first corollary concerns motive force. The (virtual) velocity V is now said to be the resultant of the motive force of the corpuscle m at a given instant and of its velocity u' in the previous instant. Evidently, then,

$$V \cos y = u' \cos z + pdt \cos x.$$

(Carnot was somewhat careless with his designations in this memoir. He identified z as the angle between u' and u, but not p as "*force motrice*" nor x as the angle between the directions of p and u. That is what he meant, however). The basic equation would then become

$$\int mu\left(u' \cos z - u\right) - \int mupdt \cos x = 0.$$

Carnot reserved to a later section (Gillispie 1971, Appendix B § 53) the proof that the resultant of several forces multiplied by the cosine of the angle that it makes with any given line equals the sum of the component forces each multiplied by the cosine of the angle that it makes with the same line. But it was in this discussion that he first employed this mode of representing and combining the directions of magnitudes of the type called vector quantities in later mechanics.

The second corollary (Gillispie 1971, Appendix B § 31) then stated that if there was impact, the term pdt became negligible compared to the value of u so that

$$\int mu\left(u' \cos z - u\right) = 0.$$

From this equation, Carnot observed, all the laws of impact could easily be deduced, although to do so would have been a digression from his purpose. For our purposes the interest lies mainly in the evidence that it was natural for him to begin with accelerative forces acting over time or distance. There was no hint of anything like a new concept to embrace that point of view. It is only what the problem involved from the outset.

Interestingly enough, a corollary about what were to become geometric motions was also couched rather in terms of force and activity than of imaginary displacements in a system. In language it resembled rather the dynamical treatment that Carnot finally gave the idea in his *Principes fondamentaux de l'équilibre et du mouvement* than the kinematical emphasis of the *Essai sur les machines en général*. It would appear, indeed, that the terms in which he thought about the problems developed from these conventional notions of force, through an intermediary stage where momentum was the subject, into the final analysis of power. The idea appeared in a way subsidiary to equilibrium. From the above fundamental equation he had derived the following consequence (Gillispie 1971, Appendix B, § 32, Corollary 3) for the case in which motion changes by insensible degrees (which he here described as that in which u differs infinitesimally from u'):

$$\int mpds \cos x - \int mudu = 0$$

(since $u' - u = -du$, and supposing $ds = udt$) or

$$\int mupdt \cos x - \int mudu = 0.$$

If equilibrium was considered to be the state of the system in which the forces p mutually "destroy" each other, this expression would reduce to the unhelpful identity, $0 = 0$. It was possible, however (Gillispie 1971, Appendix B, § 34, Corollary 5), to look at the situation in another way, and to conceive that there had been imparted to all parts of a system in equilibrium "motions such that no change resulted in the reciprocal action of the different parts of the system."

These would be motions that would be received "without alterations" (*Ibidem*), by the bodies on which they were impressed. The forces p would remain in mutual equilibrium. It would then be perfectly feasible to imagine these motions acquired in time, and the case of equilibrium would be assimilated to one in which the system changes by insensible degrees, where the equation for that case,

$$\int mpds \cos x = 0,$$

was not a mere identity. Though still unnamed, geometric motions were born of this passage and of Carnot's proclivity for thinking even of equilibrium as a state of opposing actions–most characteristically backing into a grasp on the concept of work by defining the situation in which forces did not work, or did merely putative work.

From that equation, Lazare Carnot next derived, in the case of systems in internal equilibrium, a series of corollaries that amounted to the conservation of linear momentum, the conservation of moment–of–momentum, the principle of Descartes, and the conservation of live force. To none did he give prior standing over the others. The first two he did not even identify in those terms. Two brief corollaries extended the discussion to elastic bodies, provided that the force of elasticity was included in the value of p (Gillispie 1971, Appendix B, § 40, Corollary 11). More interesting, they extended also to fluid bodies, which it was legitimate to consider as if composed of hard or elastic corpuscles (*Ivi*, § 41, Corollary 12). In a long digression (*Ivi*, § 42) Lazare Carnot derived the hydrostatical principle of equal pressure in fluids from his equation, and came finally to what his mind always naturally fixed on, the operation of machines handling weight, whether as load or source of power. After all, raising weights was the purpose of the greater number of all machines actually used (Gillispie 1971, Appendix B, § 43, Corollary 13).

It becomes immediately apparent that these were the considerations from which Carnot's approach took its distinctive features. Designating by

M	the total mass of the system,
H	the height from which its center of gravity falls,
t	the time of fall of the center of gravity of the system,
V	the velocity a body acquires in falling the height H,
h	the distance which a molecule m falls in time t, and
K	the initial velocity of the molecule m (although Carnot overlooked identifying this),

then since $ds \cos x = dh$, the basic expressions become

$$\int mpds \cos x = \int mpdh = MpdH$$

(the first summation is of individual molecules). Thus

$$2 \iint mpds \cos x = 2 \int MpdH = 2MpH$$

(the second summation is of elements of path), but

$$2MpH = MV^2,$$

and, therefore,

$$\int mu^2 = \int mK^2 + MV^2.$$ [5]

[5]Gillispie (1971), Appendix B, § 43, Corollary 13.

That elementary sequence illustrates very simply the arithmetical reasons that 50 years later led Coriolis to equate the newly named quantity work to one–half the live force, so that still later kinetic energy became defined as half the product of mass times velocity squared.

Thereupon, Carnot drew out as a passing consequence the principle that he was to propose for a fundamental axiom in the *Essai sur les machines en général*: in order to prove that the weights applied to a system are in equilibrium, it sufficed to show that the center of gravity did not descend. Now followed the remark that this principle could be applied to any machine by the device of exchanging a weight suspended over a pulley for any other force. Suppose that besides weights, other powers should be applied to the machine in order to move them, say manpower or horsepower.

The general equation would then become

$$\int mu^2 = \int mK^2 + 2 \iint mpds \cos x.$$

That last term, in turn, might be decomposed into two parts, of which one was MV^2, and the other was

> [...] double the sum of the products of each of the moving forces [i.e., the additional powers] multiplied by the element of path that it describes in the direction of that force.[6]

If F represents one such force, and u its velocity, then

$$\int mu^2 = \int mK^2 + MV^2 + 2 \iint Fudt.$$

Here was the germ of the work concept, therefore, and right alongside it that of a generalized process. Suppose that all bodies in the system were at rest at the beginning and end of the movement. In that case $u = 0$ and $K = 0$, and the expression became

$$2 \iint Fudt + MV^2 = 0,$$

or if H was the height to which the center of gravity was to be lifted, then the relation might be expressed

$$\iint Fudt = MpH,$$

[6]Gillispie (1971), Appendix B, § 46, Corollary 16.

which exhibited that in order to raise the center of gravity of a system of bodies to any height it was necessary that the quantity *Fudt* always be the same, whatever the route chosen, whatever the machine utilized, whether the parts of the system be raised separately or together, and whether a single force or several together be employed[7] (Gillispie 1971, Appendix B, § 47, Corollary 17). A further passage (*Ivi*, § 49) identified the factors, F, u, and t to be varied in achieving mechanical advantage and gave examples of processes in which it would be prudent to economize one factor or another. Carnot observed that it would result in a saving of force if each part of the system completed the process with zero velocity, or at least with as small a velocity as possible.

The second and third sections, comprising § 51 through 85, of the theoretical part of the memoir dealt respectively with equilibrium and with motion in the case of the seven classes of simple machines. Actually, Carnot remarked at the outset, "cords" (*Ivi*, § 51) and the lever were the only truly simple machines. It was even possible to reduce the former to the latter, but seven types of machines were customarily considered to be simple: cords, the lever, the pulley, the crank, the inclined plane, the screw, and the wedge. His treatment adapted his equation, $\int mpds \cos x = 0$, to the characteristics of each in turn in order to state its law of equilibrium while "[. . .] taking account of friction and the stiffness of cords as far as I could [. . .]" (Gillispie 1971, Appendix B, § 49). The expressions were artificial and the discussion forced, and perhaps that was its most important feature since the central purpose of the *Essai sur les machines en général*, a few years later, precisely was to achieve a general science of machines and to obviate the necessity for deriving laws of equilibrium and motion for each particular type of machine. The third section on motion was more summary, though no clearer, and ended with an appeal to the possibility of combining his principle with that of d'Alembert in order to reduce all questions about the theory of machines to problems of analysis.

Certain features of this discussion, however, did hold importance for Carnot's later work. He devoted the fullest treatment to the law of equilibrium for the "funicular machine" (*Ivi*, § 52), and it was there that he introduced his convention for projecting forces (he did not then speak of other magnitudes) upon other directions in terms of the cosine of the angle, balanced the moments of the resultants of clockwise against counter clockwise forces, and finally considered the projection of the machine onto a plane in which the projection of the applied powers became a two–dimensional force system. (These matters had so central a part in Carnot's later work that it seems wise to extend the reproduction of the theoretical portion of the 1778 memoir in order to include them (*Ivi*, §§ 52–60). It would seem that Carnot's thinking about vector quantity may have originated in his mental picturing of the distribution and transmission of forces by the cords of machines worked by ropes).

In summary, it may be said of this first memoir that Carnot had in it the germ of his central ideas, but not as yet differentiated from a central principle of

[7]It would be perfectly possible to see embedded in this Carnot's principle of continuity, but there is no evidence that he then saw there.

equilibrium into concepts themselves. He merely alluded in passing to the well–known principles of mechanics, such as conservation of momentum or conservation of live force, without emphasis on one as more relevant than others to his purposes. If any single concept may be called fundamental for him at this stage, it would be force in the sense of motive force or pressure, but only tacitly so–it was no more than what he meant when he alluded to force. Missing (except as fugitive hints or questionable clues) were two of the central features of the *Essai sur les machines en général*, the distinction between impelling and resisting forces and the notion of a generalized science of machines to be disengaged from the application of laws of mechanics to the classes of machines taken successively. The notion itself was not there, much less the recognition that the way to bring it to pass was to treat machines as themselves inertial bodies endowed with mass. (Seeing that is what impelled him in the *Essai sur les machines en général* to go over to terms of displacement and work.) It would be fairly farfetched, though perhaps just barely possible, to say that one discussion that took into account the weight of the lines in a funicular machine represented the first hint of what Carnot later developed into the main emphasis of his theory of machines (Gillispie 1971, Appendix B, § 60). It would be more reasonable to imagine that it was writing on theory of machines while repeatedly taking account of friction, instead of neglecting it, that led Carnot into the way of considering friction (like the weight of the machine) in the light of a force against which the motive forces had to work. In the concluding passage to Section I of the theoretical part Carnot said expressly that the force of friction and other "resistances" (*Ivi*, § 50) might be considered as "active" forces (*Ibidem*).

3.2 Argument of the 1780 Memoir

In reopening the subject for an award in 1781, the Academy reiterated its phrasing of the problem except that it now specified that the friction to be investigated should be that between the moving parts of simple machines:

"The theory of simple machines with regard to the friction of their parts and the stiffness of cords [. . .]" (Carnot 1780, Appendix C, in Gillispie 1971). A note in Carnot's own hand on a cover sheet indicates that he completed it at Béthune on 15 July 1780. In resubmitting his memoir, anonymously as was the Academy's practice, he identified it by the same motto from Titus Lucretius Carus (ca. 99 BC–ca. 55 BC) and observed in a note that having been accorded an Honorable Mention in the first competition, he had now "diminished its imperfections as far as I could", *Mémoire sur la théorie des machines* (Gillispie 1971, Appendix C, p 299).

In fact, he had enlarged and transformed it into a memoir that in important respects is the draft of the *Essai sur les machines en général* of 1783 even to the wording of many central passages. Indeed, comparison of the two prize memoirs makes it apparent that, though the genesis of Carnot's mechanical thought belonged to his education and to the years at Calais and Cherbourg, its formative period came between his first failure to win the prize (the announcement was made at Easter

1779) and the completion of the revised memoir in July 1780. During the interval of approximately a year he must have been reading, thinking, and living little other than the theory of machines.

True, he was still bound by the terms of the Academy's contest, and the organization followed that of the earlier memoir. The 1780 memoir also opened with experiments in Part I and went on to a Part II pretending to be their application to practice. The latter again consisted of two sections, one on "machines in general" and a conclusion on laws of equilibrium and motion in each class of simple machine. In deference (no doubt) to the Academy's predilections, Carnot did elaborate and complete his program of experiments. He introduced it with a broad discussion of friction in general, and this time classified the experiments according to whether the bodies touched point–to–point, line–to–line, or surface–to–surface. He devised new experiments, improved the account of the old ones, and dropped the most cumbersome of the formulas for variation of friction with velocity. Instead, he substituted a tabulation of coefficients of frictional interactions between certain actual materials: iron, copper, red chalk, beechwood, ash, yew, and elmwood. It remains difficult, nevertheless, to see that any of this material, or any of the succeeding section on cordage, had a life longer than that of this memoir, which there is no evidence that anyone ever read again between the time that the Academy awarded the prize instead to Coulomb and the writing of the present study. The attempt left Carnot himself without illusions. The concluding paragraph to the experimental part reads:

> I do not, therefore, conceal from myself that a great deal remains to be done before the knowledge both of friction and stiffness in cordage leaves nothing more to be desired. If someone has given a fully satisfactory account of the topics I have just considered, he will have rendered a great service to practical mechanics, and certainly so difficult a task could be worthily rewarded only by the recognition of the Academy.[8]

As was to be expected, therefore, the interest lies again in the first section of Part II, which consists of the second version of his theory of machines. The discussion was lengthier than in the first memoir, 60 paragraphs occupying some 40 folio pages, though still brief compared to the *Essai sur les machines en général*. In it he achieved notable modifications–in clarification of topics, in the enunciation of principles and definition of quantities, and in the articulation he had given to what the historian can now recognize to be a new mode in the science of mechanics and not merely the groping of a young engineer with a rather special set of interests and a point of view still latent. Indeed, without needing to speak of discovery in the sense of things or relations altogether strange and unknown, we may be justified in taking this text for the most reliable index to the aspects of Lazare Carnot's work that were most characteristically his own. The year since the failure of his first memoir was not sufficient for him to have read extensively in the literature or to have mounted elaborate experiments. It did suffice, however, for him to see through his problems, to seize their generality, and to elaborate his point of view in coherent propositions.

---

[8] Gillispie (1971), Appendix C, § 100.

Resubmitting his memoir to a jury of the Academy, he might have been expected to put into it that which would have reflected most credit on his capacity to contribute to the subject. On the other hand, he was not then writing, as he tried to do a few years later in the *Essai sur les machines en général*, for extrascientific readers for whom his own findings needed to be embedded in elementary exposition of the basic commonplaces of mechanics itself.

On the hypothesis that what was in the 1780 memoir, therefore, was largely Carnot's own, let us see what the arrangement and development of the main motifs were. First, he specified the subject to be the properties common to all machines and began the discussion, not in medias res as in the first memoir, but with the operational derivation of the center–of–gravity principle for equilibrium in a generalized machine, the same with which he later, in the *Essai sur les machines en général*, repaired the deficiency he there exhibited in the traditional statement.[9] As for the so–called Principle of Descartes, Carnot here stated it in passing as a consequence without identifying it by that or any name, and went on to assure the generality of the center–of–gravity principle by remarking that all forces could in principle be reduced to the action of weights.

A brief and clear diagnosis followed of the impediment that mechanics had created for the science of machines in abstracting from their mass. Let machines be considered instead as inertial bodies, and their science like that of all mechanics would be reduced to the problem: given the virtual motion of a system of bodies, what actual motion would ensue in consequence of their mutual interactions? The problem thus posed simply and directly, so were the conditions for a solution. The first assumption, though not so stated, was the comparative anatomy of hard body and elastic body. The second was the sole principle that Carnot always held to have been requisite for a resolution: the equality in opposition of action and reaction, a law simple, "incontestable" (Gillispie 1971, Appendix C, § 111) and of universal applicability.

On that basis alone Carnot proceeded to derive the two fundamental Equations (E) and (F) (see Eqs. 2.1 and 2.2) of the *Essai sur les machines en général.*[10] The wording was identical except that the latter he here called an arbitrary rather than an indeterminate. In the *Essai sur les machines en général* he corrected himself and held that it was the values to be attributed to the geometric motions *u* of Equation (F) that were arbitrary, not the relation itself. More striking are passages adumbrating the concept of geometric motion, identical in phrasing to those in the *Essai sur les machines en général*. Their role was that of auxiliaries allowing the derivation of the general indeterminate (F) from the determinate (E) restricted to elastic interactions. Only one feature of the later introduction of geometric motions was missing, the afterthought of the *Essai sur les machines en général* wherein Carnot included in

[9] See above Chapter 2, pp 20–22.

[10] See above Chapter 2, pp 25–28.

the class of geometric motions those that, while involving no virtual displacement, did no work.[11] At first reading, it is surprising that Carnot should not have seen the need for that provision. It seems closer to the dynamical emphasis that foreshadowed the concept of geometric motions in the 1778 memoir[12] than does the kinematical way in which he introduced the idea explicitly here and in the *Essai sur les machines en général*. Nevertheless, the final development he gave the notion in the *Principes fondamentaux de l'équilibre et du mouvement* of 1803 was a protoenergetic one. It may be, therefore, that the successive installments on geometric motions exemplify three phases through which Carnot's thinking about the whole subject passed. In the first phase, that of the 1778 memoir, it was motive force that was primary; in the second, that of the 1780 memoir and much of the first part on mechanics of the *Essai sur les machines en general* it was displacement; and in the third, that of the latter part of the *Essai sur les machines en général* concerned with machines and of the *Principes fondamentaux de l'équilibre et du mouvement*, it was power or its exercise in moment–of–activity or work.

Such must almost surely have been the order in which he evolved the science of machines, for comparison point by point of the 1780 memoir with the *Essai sur les machines en général* shows that in the latter published version he superimposed the more theoretical work passages and principles upon the arguments of the unpublished memoir without rethinking or reshaping them. The contrast comes out most strikingly in the absence from the memoir of the feature that Carnot added to and emphasized most strongly in the *Essai sur les machines en général* the conservation of the moment–of–the–quantity–of–motion, or moment–of–momentum. That principle was there introduced, it will be recalled, in a curiously repetitive manner. First, the conservation of angular momentum appeared unnamed and merely as a sample solution to the problem resolved by employing the indeterminate Equation (F) (see Eq. 2.2). It would be reasonable to suppose that in that form it represents the early phase of Carnot's thinking. Then there followed in the *Essai sur les machines en général* (but not in the 1780 memoir) the digression just recalled about the pseudogeometric status of motions that do no work, upon which Carnot came back to conservation of the moment–of–the–quantity–of–motion now magnified and. elevated to the status of the fundamental conservation law. Stating it in the form of three propositions,[13] it will be further recalled, he went on to derive from it well–known theorems of mechanics followed by his own equation designated (AA) (see Eq. 2.3):

$$\int Fu \cos Z = 0,$$

[11]See above Chapter 2, pp 19–23.

[12]See above Chapter 3, p 54.

[13]See above Chapter 2, p 30.

in which F, representing force in the sense of motive power, was substituted in effect for the product mU, the quantity of motion "lost" to internal interactions in the so-called fundamental Equation (F)

$$\int muU \cos z = 0.$$

Not so in the 1780 memoir, which said nothing of moment–of–the–quantity–of–motion. Having defined geometric motions in point of displacement only, Carnot simply obtained Equation (F) for the general solution and expressed it in various ways adapted to different needs. One proposition resolved the quantities into Cartesian coordinates. A second exhibited the analogy to conservation of live force in hard body interactions and included a term for its loss in impacts. A third concerned motion changing by insensible degrees over time and reverted to the expression Carnot had obtained in the 1777 memoir, i.e.,

$$\int mupdt \cos z = \int mud\ (V \cos y).$$

A fourth concerned the case in which the value of u is the real velocity V and found that the expression then reduced to conservation of live force. All these consequences came out rather parenthetically, however. Carnot's discussion in the 1780 memoir placed no emphasis on the principles of mechanics and made much less of the later claim that geometric motions permitted transforming live–force conservation in elastic bodies into an expression applicable to generalized hard–body interaction. These matters figured here rather as a sequence of problems than a deductive chain of principles, for it was a feature of the way in which Carnot's ideas developed that he tended in later work to dress up in the guise of principles what he had initiated as the solution to problems.

Also missing in the 1780 memoir was the analysis of power which in the *Essai sur les machines en général* made the transition between Part I on the principles of mechanics and Part II on Carnot's own subject, "machines strictly speaking". The omission is consistent, for it was in order to prepare the ground for those considerations that Carnot would then introduce the moment–of–the–quantity–of–motion into the *Essai sur les machines en général* in the interval between 1780 and its publication in 1783. All these differences, therefore, reflect the limitation on his thinking at the time of the 1780 memoir: there at the point where he was later to move on to the concept of power and work, he fell back on the traditional notion of dead force (Gillispie 1971, Appendix C, § 128). When the question was one of live forces applied to machines, he observed, the theory was related to that of impact, which sufficed to solve such problems. But the theory of machines conventionally understood envisaged only the forces employed to move them, i.e., dead weights, animal power, and wind or water. Accordingly he would limit himself to these forces for the remainder of the theoretical part of his memoir.

Nevertheless, limited though Carnot's vision may thus have been at the stage of the 1780 memoir, the evidence is interesting in showing that it was in connection both with such conventional problems and also with the later, more elaborate notions that he developed the concept of work. For if he did not have it fully disengaged in 1780, neither was it entirely missing. The quantity itself, moment–of–activity in 1783 in the *Essai sur les machines en général*, was called "quantity of action" in the 1780 memoir. There it was always coupled with impelling or resisting forces (for those terms also made their appearance) as the quantity that they produced or consumed. Carnot never referred to it without the qualifier and expressly warned the reader against confusing his quantity of action consumed or produced with that which mathematicians following Maupertuis meant by quantity of action of a system. Of action in that sense he, Carnot, would make no use in the present memoir (Gillispie 1971, Appendix C, § 132), and it must have been the obvious inconvenience that led Carnot to modify the terminology in the *Essai sur les machines en général*. It may, further, have been the emphasis there placed on moments in general, particularly that of the quantity of motion, that led him to redesignate his basic quantity "moment–of–activity" and to consider it in itself and not merely as a measure of the impelling or resisting forces it was consuming or producing.

Lacking a fully disengaged concept of power or work, together with the principle of conservation from which to deduce it, Carnot gave the sequence of propositions about machines themselves more episodically in the 1780 memoir than in the *Essai sur les machines en général* and failed to draw out of them his own principle of continuity, which appeared only in the form of a practical injunction in passages constituting the draft of the later scholium. In the *Essai sur les machines en général*, it will be re–called, he stated a single theorem for machines:

> Whatever the state of rest or motion of any system of forces applied to a machine, if a geometric motion be imparted to it without altering the forces, the sum of the product of each by the velocity of the point of application, evaluated at the first instant and in the direction of the force, will be zero.[14]

Or

$$\int Fu \cos z = 0,$$

from which he drew all the corollaries comprising the science of machines "strictly speaking", the fifth being what we have called the Work Corollary:

> *In a machine of which the motion is changing by insensible degrees*, the moment–of–activity consumed *in a given timeby the impelling forces* equals the moment–of–activity exerted *in the same time by the resisting forces*.[15]

---

[14] «THÉORÈME FONDAMENTAL. Principe général de l'équilibre & du mouvement dans les machines. XXXIV» (Carnot 1786, p 68).

[15] Carnot (1786), § XLI, pp 75–76.

The 1780 memoir had yet to achieve that unification of his point of view. In the absence of conservation of the moment of the quantity–of–motion, and therefore of work, he stated two parallel theorems for the science of machines, the first for equilibrium and the second for motion. Thus the former:

> When a machine is in equilibrium, if an arbitrary geometric motion be imparted to it without altering the applied forces in any way, the quantity of action produced in the first instant by the impelling forces will be equal to the quantity of action produced in the same infinitely short time by the resisting forces.[16]

That proposition he then stated in various ways (one of which is virtually identical to the phrasing just cited from the *Essai sur les machines en général* except that it did not embrace a machine in motion) and drew from it several corollaries. In those concerned with weight-driven machines, the equivalence of MgH to $\frac{1}{2}MV^2$ came out repeatedly.

The companion theorem concerning machines in motion was stated more awkwardly:

> If the actual motion of a machine is suddenly converted into any other geometric motion and the machine is left to itself, the conservation of live forces will obtain throughout the ensuing motion no matter what changes there may be in the motive forces.[17]

The corollaries with which Carnot followed this proposition were all correct, and the second of them essentially amounted to the Work Principle of the *Essai sur les machines en général* in the following form:

> When a machine is in uniform motion (i.e., when each point of the system has a constant velocity) the quantity of action produced in a given time by the impelling forces is equal to the quantity of action produced in the same time by the resisting forces.[18]

The statement was weaker than in the *Essai sur les machines en général,* but already Carnot identified it as the most useful of all the propositions in the theory of machines. The historical question that poses itself, therefore, is what difficulties still remained in his mind that prevented his having combined motion and equilibrium into a single principle as he seemed quite naturally able to do in the *Essai sur les machines en général*?

Several possible explanations suggest themselves. One is that lacking moment–of–momentum and thinking in terms of displacements, he had not seized on all the advantage that his geometric motions conferred in authorizing him to neglect internal forces of the system. That might be why he followed the theorem with this otherwise unnecessary justification–since live forces were conserved in a

---

[16] «*Théorème 1er*. Principe général de l'équilibre dans les machines» (Gillispie 1971, Appendix C, § 133).

[17] «*Théorème II*. Principe général du mouvement dans les machines» (Gillispie 1971, Appendix C, § 140).

[18] «*Corollaire II*. Des machines qui se meuvent uniformément» (Gillispie 1971, Appendix C, § 143).

system of which the motion was changing by insensible degrees, and since when a geometric motion was substituted for a real one, nothing was changed in the mutual interactions of the system, it followed that motion must still be changing by insensible degrees, and hence live forces must still be conserved "[. . .] at least for some time [. . .]" (Gillispie 1971, Appendix C, § 140) after such a transformation, no matter what motive forces should be substituted for those that were really influencing the parts of the system.

Another possibility is that Carnot had not quite succeeded in generalizing in his own mind relations that he had obtained from considering the behaviour of weights under gravity. That might be why the corollaries are clearer than the statement or discussion of the theorem itself, notably the first which concerns weight–driven machines:

> If the actual motion of a weight–driven machine be suddenly changed into any other geometric motion whatever, and the system be left to its own forces, the ensuing sum of the live forces at any instant is equal to the sum of the initial live forces (i.e., immediately after the change of motion) plus what the sum of the live forces would be if each point of the system had a velocity equal to that due to the height from which the center of gravity has fallen since the change of motion.[19]

Perhaps, finally, the obligation to conduct the argument toward a frictional application concealed the desirability of establishing the work principle in a unified treatment of motion and equilibrium. The reason he advanced in the 1780 memoir for considering it the most useful for the theory of machines was that in practice such ordinary devices as mill–wheels, pulleys, and capstans, once set in motion, soon reached a steady rate of operation. A final corollary brought all real machines within the scope of the discussion by specifying that friction was always to be classified as a resisting force, since its direction inevitably countered the real motion of the points whereon it acted.

It would be misleading, however, to emphasize shortcomings on the formal level of argumentation, and it is more illuminating historically to appreciate that what Carnot lacked as principle in the 1780 memoir, he already had as maxim. The final paragraphs of the theoretical section of the 1780 memoir were largely similar and in certain places identical with the *scholium* that concluded the *Essai sur les machines en général.* An interesting passage reveals the latency of his point of view. He had reverted to machines of which the state of motion changes by insensible degrees and pointed out that they had in common with periodic machines (i.e., pendulums) that live force was conserved and that in a given time, therefore, the quantity of action produced by the impelling forces equals that produced by the resisting forces. Even worthier of remark, a quantity one–half the increase in the sum of the live forces is identical with the quantity of action produced by the force of inertia of the bodies comprising the system. It followed that:

> [. . .] if we consider all forces, whether active or passive, applied to a machine in motion, including even the inertia of its matter taken itself as a real force that resists change of

[19] «*Corollaire I^{er}*. Du mouvement dans les machines a poids» (Gillispie 1971, Appendix C, § 142).

> state in the bodies, the quantity of action produced by the impelling forces in a given time will always equal the quantity of action produced in the same time by the resisting forces whatever be the motion of the machine [. . .].[20]

Here then, noted in passing in the 1780 memoir, was precisely the remark that he stated rather as a theorem in the *Essai sur les machines en général*, and that there appeared to be the principle in virtue of which he unified the discussion. Indeed, he even noted in the memoir that the entire theory of machines whether at rest or in motion could be reduced to that single statement. He simply did not yet do it, which shows that what he really needed to develop was less the content of theorem or propositions than the line of reasoning that connected them.

As for his own principle of continuity, it followed upon a discussion virtually identical with that in the *Essai sur les machines en général* about varying the factors of time, force, and velocity in achieving mechanical advantage. A footnote distinguishes between "[. . .] absolute work or labor of the agent [. . .]"[21] and "what I call here the work of the moving or impelling force [. . .]" the difference residing in the extra force consumed by the inertia of the machine itself. The things that he mainly had in view in all these remarks were the classical simple machines and their variants used on shipboard. It was for this reason, Carnot said further, that he had written only about those of which the motion changes by insensible degrees.

> For when the purpose is to produce the greatest effect possible from a machine, there is a decisive advantage to be gained in excluding percussion or sudden changes in the state of motion.[22]

Carnot went on to show how the intervention of impacts necessarily involved loss of a corresponding quantity of action. The argument was in much the same terms as that reproduced 3 years later in the *Essai sur les machines en général*, but the mode in the 1780 memoir was one of explanation rather than demonstration or proof (Gillispie 1971, Appendix C, § 157). It is, indeed, the most revealing feature of the document that this point, which later appeared to be the chief finding of Carnot's mechanics, so much so that it was called his law or principle, he first wrote down not as discovery of an unknown truth, but as a justification for limiting his subject to machines that in their operation embodied it.

For the rest, the 1780 memoir presents little interest. Like that of 1778 it concluded with a section purporting to apply the experimental results to determining expressions for each class of simple machine. Carnot had simplified the treatment over that of the earlier memoir by combining consideration of motion and equilibrium in each class of machine–all the more curious does it seem, therefore, that he did not do this in the preceding theoretical discussion–rather than by going through all seven classes once for the case of equilibrium and again for that of motion. He

[20] «*Scholie*» (Gillispie 1971, Appendix C, § 148).

[21] «[. . .] le travail absolu ou la peine [. . .]» (Gillispie 1971, Appendix C, § 153, footnote).

[22] Gillispie (1971), Appendix C.

recognized that his organization had the effect of separating the theoretical from the practical study of machines in general. No doubt the terms of the Academy's competition imposed on him the necessity thus to descend to particular cases for the practical part. But it does not appear that he had formed the project of such a science of machines as would dispense him from specifying particular conditions of motion and equilibrium characteristic of each class in practice. The separation of theory and practice was necessary, he observed, "[. . .] in order to avoid the confusion that would arise from mingling them [. . .]" (Gillispie 1971, Appendix C, § 191).

We can see that he had in mind with greater or less cogency all the relations or theorems that would comprise such a science. What he had still to accomplish was to graduate in his thinking from forces to powers and to seize on the conservation of the moment–of–momentum in order to make full use of his own idea of geometric motions in abstracting from the effects of internal forces in a system and hence overcoming the disjunctions between hard and elastic body, pressure and impact, dead force and live force. In a way that was conceptually subsidiary but probably more important in actuality, he needed to generalize from the manipulation of weights, whether as load or power, in order to embrace within his reasoning all processes involving work.

Such, at least, was the development that he gave his subject between the failure of his second memoir to win the academic prize in 1781 and the publication in 1783 of his *Essai sur les machines en général*, in which all the experimental and particular passages were eliminated, and the theory of machines alone was presented rather as an adaptation of the science of mechanics itself. It must, therefore, have been during this interval that he reviewed the whole science as it was known to an engineer in the late eighteenth–century, though how and through the agency of precisely which writings in that science he then seized on the unifying possibilities inherent in the principle of moment–of–momentum, the sources do not permit us to say: for the rest of it, all the development he gave was latent in his own approach in the two memoirs successively submitted to the Academy.

3.3 Argument of the *Principes fondamentaux de l'équilibre et du mouvement* (1803)

The remaining discussion of Carnot's science of machines will concern its immediate development and influence. Let us take first the revision that he himself gave the subject in his *Principes fondamentaux de l'équilibre et du mouvement* (Carnot 1803a) published just 20 years after the *Essai sur les machines en général* and in the same year with the *Géométrie de position* (Carnot 1803b). Most of the later writers who allude to Carnot's mechanics make reference to the *Principes fondamentaux de l'équilibre et du mouvement* rather than to the *Essai sur les machines en général*, which, if mentioned at all, is not always distinguished from the

second, more renowned version.[23] Meanwhile, the development of the eighteenth–century French school of rational mechanics had culminated in the publication in 1788 of Lagrange's *Mécanique analytique*, a work that imposed itself immediately as the mathematical chef d'oeuvre of its subject. Lagrange's book was no isolated monument. On the contrary, the last two decades of the eighteenth century were a period of intensive activity in the formalization of mechanics, so much so that Carnot himself remarked in the preface to his *Principes fondamentaux de l'équilibre et du mouvement* that in the interval since his earlier writing, there had appeared so many others "[. . .] on all aspects of mechanics, so beautiful and so extensive that my own can scarcely be remembered" (Carnot 1803a, p v). (So far as can be determined, his modesty was accurate. The present writer has never seen a single mention of the *Essai sur les machines en général* in the contemporary literature of mechanics, nor any evidence that it was ever read).

Why, then, was a second edition wanted? Carnot justified it on the grounds that the *Essai sur les machines en général* had contained some ideas that were new when they first appeared, and that it was in any case always useful to envisage the fundamental truths of science from various points of view. Of the new version he said further, "[. . .] several scientists have strongly urged me to furnish it" (*Ibidem*). Unfortunately, we do not know who they were. Did they do so because Carnot was now a prominent statesman recently emerged from political eclipse and influential in the Institute? It would be unrealistic to suppose that such facts carried no weight, although neither is it necessary to impute base and much less baseless flattery to Carnot's advisers. Statesman though he now was, he was a statesman with a certain scientific reputation in virtue of his mathematical writings. He had published his book on the calculus in 1797 and his first book on geometry in 1801.

Are we to suppose, then, that the new engineering emphasis in technical education might have been thought to create a public for a book on mechanics from the point of view of a former engineer, since become famous? So it almost certainly was in Carnot's own mind, for the *Principes fondamentaux de l'équilibre et du mouvement*, unlike the *Essai sur les machines en général*, is at once textbook and treatise in the fashion created by the *École polytechnique*. The former quality flattens Carnot's already unassuming style and puts the book in the shadow when contrasted to the brilliant light in which the works of Lagrange and his school are illuminated by their formal elegance. At all events it must have been some such combination of personal and institutional motivations that encouraged Carnot to revise his science of machines. The explanation cannot be that his approach was

[23] See, for example, the following works: *L'entropie. Son rôle dans le développement historique de la thermodynamique* by Brunold (1930, pp 37–38), *Some factors in the early development of the concepts of power, work, and energy* by Cardwell (1967, pp 209–224), *Histoire de la mécanique* by Dugas (1955, pp 324–331) and *Lectures de mécanique. La mécanique enseignée par les auteurs originaux* by Jouguet (1924, II, p 72).

an answer to the problem structure in the science of mechanics itself, for (as will appear) if his work was no longer unknown, it was another 20 years before these concerns began to make any difference.

To turn to the content of the *Principes fondamentaux de l'équilibre et du mouvement*, it will be convenient to take it in its textbook aspect first, for Carnot had completely reorganized his presentation. The division between fundamental principles of mechanics and the theory of machines "properly speaking" had disappeared. The subject was still presented in two parts, but the distinction was now between its experiential and its rational aspects. The former reads like a glossary of mechanics. It comprised mainly definitions of the quantities and expositions of elementary laws of statics and dynamics for which Carnot claimed the sanction of experienced fact. Part II by contrast contained propositions that could be derived from those first principles merely by ratiocination–of them more in a moment, for they were those that were peculiarly Carnot's own either in conception or in function.

In observing that Part I now reads like a primer, the historian should intend nothing denigratory. It was a good primer. No franker source exists if he wishes to know what persons literate in mechanics at the end of the eighteenth century understood physically by its basic conceptions, such as force, motion, action, velocity, acceleration, etc.[24] Like many of the simplest matters generally taken for granted, these are not always easy to come by historically. In this respect, the *Principes fondamentaux de l'équilibre et du mouvement* exhibits a notable improvement in clarity over the *Essai sur les machines en général*, in which the modern reader often has to judge of Carnot's comprehension of terms and dimensions by the use he made of them rather than by an explanation. In the later work, for example, the discussion of projection and combination of directed quantities was explicit. It is extremely curious, however, that Carnot seems to have gone all the way back to his earliest, 1778, memoir for these proto–vectorial considerations[25] (Gillispie 1971, Appendix B, § 52–54). Was it his intervening work in geometry that encouraged him to see the merit of this representation, which he had merely taken for granted in the intervening work in mechanics? It may be so. At all events Carnot suggested the following notation for representing the projection Aa' of one velocity Aa upon an intersecting straight line AB:

$$\overline{Aa'} = \overline{Aa} \cos \overparen{\overline{Aa} \quad \overline{AB}},$$

wherein the last term denoted the angle, a convention that would have been obvious although cumbersome if generally adopted. Here, for example, is his expression for

[24]Please see above Chapter 2, p 19, ft 7.

[25]The geometric figures missing from the 1778 Ms. may be reconstituted from Figs. 2, 3, and 4 of Pl. I of the *Principes fondamentaux de l'équilibre et du mouvement,* which fit the argument of the 1778 memoir.

the relation between initial velocity ($\overline{MW}$), altered velocity ($\overline{MV}$), and the velocity ($\overline{MU}$) "lost" when a body was constrained to change its motion[26]:

$$\overline{MW} = \overline{MV} \cos \overline{MW}\widehat{\quad}\overline{MV} + \overline{MU} \cos \overline{MW}\widehat{\quad}\overline{MU}$$

Writing of moments, Carnot now pointed out that the moment of a motive force, being the product of a force by a line, could always be reduced to a live force, and that the moment of a quantity–of–motion could be reduced to the product of a live force and a time. He now had clear that the latter was properly designated quantity–of–action of a mass. The concept of work he still called moment–of–activity, but his way of seeing these quantities seems to represent a certain development in the differentiation of the notion of energy (live force) from that of work (moment–of–activity). He distinguished between moments–of–activity "consumed" or "absorbed" (instead of consumed or produced). By the former he still meant the product of the force by the path described by its point of application, evaluated in the direction of the force; whereas the latter was now the product of the force by the velocity of the point of application, evaluated in the direction opposite to that of the force. It still comes out that the two quantities are identical numerically and of opposite sign since the angles are supplementary, but the distinction seems closer than it had done in the *Essai sur les machines en général* to that in later usage between the work done on or by a system (Carnot 1803a, §§ 60–61, pp 38–39). For there is the beginning of a distinction here between notions that in the next 50 years would evolve into the concepts of kinetic and potential energy. He laid it down that live force "properly speaking" had the dimensions MV^2 (as we would write it) and that when it took the form of the product of a motive force by a line (*PH* as he wrote it, or *mgh* as we would write it), it could be given the special name of "latent live force" (*Ivi*, § 64, p 41). In the latter case it was dimensionally identical with moment–of–activity (or work), and the significant thing is that Carnot saw it as conceptually distinct. For a system of bodies in which forces were operating over time, the following expression

$$mp \cdot udt \cos k$$

would give the moment–of–activity consumed during time *dt* by the force *mp* with respect to the velocity *u* (*k* being the angle between *u* and P). The moment–of–activity consumed by the whole system throughout the entire motion would then be given by

$$\sum \int mp \cdot udt \cos k$$

[26]For the Eq., see Carnot (1803a), § 43, p 26.

(of which expression one might note that Carnot had begun to distinguish between summation and integration, and also that he had lapsed into his old notation). That expression being a latent live force, it could be reduced to a quantity of the form MV^2:

> Whence it is easy to appreciate that the notion we have just given of moments–of–activity is encountered frequently in the theory of equilibrium and motion, either in the form of live force properly speaking, or in that of latent live force.[27]

From the definitions Carnot turned to a summary of what he now called "The hypotheses that can be admitted as general laws of equilibrium and motion" (Carnot 1803a, p 46), which he pretended to found on experience and reasoning, and which were in fact simply the classical laws of statics and dynamics. It would elucidate nothing not already evident about Carnot's reasoning to enumerate these propositions. Two remarks only may be curious to note. The first reflects on Carnot's own sense of historicity. He here attributed the principle of virtual velocities to Galileo, even calling it the "principe de Galilée", and went on to credit the lowest center of gravity condition for equilibrium to Torricelli as its consequence before rectifying it in his own operational statement. The attribution to Torricelli was in fact correct, though not that to Galileo, and one has the impression that even in the rather trivial respect of acknowledging the history of the concepts, Carnot's reworking of the book was shaped by the example of Lagrange. The second remark about Carnot's presentation of the elements is that he concluded it by deriving from the conservation of live force the relations that he needed in the various cases of change of motion. As usual, W was initial, V final velocity, and U the velocity lost in impacts. Then for hard bodies in the general case,

$$\int MW^2 = \int MV^2 + \int MU^2,$$

for hard bodies in which the motion changes by insensible degrees,

$$\int MW^2 = \int MV^2 \quad \text{(since } U \text{ is infinitesimal)}$$

For elastic bodies, he comes at it in reverse order. Since

$$\int MV^2 = \int MW^2 - \int MU^2,$$

and since the velocity U lost at impact is restored on rebound,

$$\int MV^2 = \int MW^2.$$

[27] Carnot (1803a), § 64, p 41.

These are statements remarkable only in that he could now thus simply set them down one after the other (Carnot 1803a, §§ 131–132, pp 104–105).

In Part II of the *Principes fondamentaux de l'équilibre et du mouvement* Carnot turned to a formal development of the so–called hypotheses considered as laws of nature. It is difficult, on the whole, to think the presentation felicitous. Ostensibly the goal was to achieve analytical expression for the laws, and the result was that the distinctiveness that Carnot brought to mechanics by directing attention to the study of machines was obscured by his attempt (or so it would seem) to imitate in some degree the style of Lagrange. The sequence of theorems swept so wide of the subject of machines that its denouement there would come as something of an anticlimax except that the climax itself, a generalization or vindication of the Principle of Least Action, was somewhat beside the point.

At the same time it would be wrong to underestimate all the theoretical aspect of the *Principes fondamentaux de l'équilibre et du mouvement* just because this part of the book may not have been an entire success or because he had already made all the significant points in the *Essai sur les machines en général*. He did achieve a greater clarity, most notably in the passages defining geometric motion:

> Any motion that, when imparted to a system of bodies, has no effect on the intensity of the actions that they exert or can exert on each other in the course of any other motions imparted to them, will be named geometric.[28]

Clearly, he had modified the definition, for he states it entirely in terms of the function of geometric motion, which it is now easier to recognize as what in later parlance would be called virtual displacements. Neither in the 1780 memoir nor in the *Essai sur les machines en général* had Carnot adapted his concept of geometric motions from the principle of virtual velocities. In the *Principes fondamentaux de l'équilibre et du mouvement*, however, he went on to recognize the analogy between such motions and that principle in the use Lagrange made of the latter. The difference was that virtual velocities being infinitesimal were inapplicable in problems involving the sudden changes of motion produced by impact, whereas geometric motions, being finite, generalized the principle so that it might apply to all cases, whether the change of motion be continuous or discontinuous, whether the bodies be hard, elastic, or fluid (Carnot 1803a, § 144, pp ix–x, p 115).

It is because of these passages that the historian will detect in the concept of geometric motion the forerunner if not the common ancestor of reversible processes and vector analysis, the former in that reversibility was the criterion by which the independence of such motions from the rules of dynamics might be recognized, and the latter in that these motions were, therefore, determined by the geometry of the system alone. Carnot nourished a prophetic if not always lucid vision of the prospect for a science that should be "[. . .] intermediary between ordinary geometry and mechanics" (Carnot 1803a, § 162, p 131). Both in the *Principes fondamentaux de l'équilibre et du mouvement* and in the *Géométrie de position* he alluded to hopes for

[28] «DÉFINITIONS» (Carnot 1803a, § 136, p 108).

thus uniting mechanics and geometry in a science yet to be created. It was mainly for lack of a theory of geometric motions that analytical difficulties frequently impeded the solution of mechanical problems. Carnot thought this to be specially true of hydrodynamics (Carnot 1803a, § 145, p 116), and the remark goes to strengthen the case for seeing in geometric motions the origin of reversible processes, since it was in connection with the fluid model of heat transfer that Sadi Carnot introduced the concept of reversibility.

As an earnest of such a theory, Carnot opened the "rational" part of his *Principes fondamentaux de l'équilibre et du mouvement* with theorems, of which the first nine about the properties of geometric motions in hard–body interactions. The sequence will prove somewhat puzzling to the reader not yet oriented to his purposes in mechanics itself. When he did finally come to that, he unfolded the argument in a manner different from that of the *Essai sur les machines en général*. Both editions prepared the ground for the application of mechanics to machines with the proposition that in the motion of a system of hard bodies, the net effect of mutual interactions of the parts is zero. In the *Essai sur les machines en général* (it will be recalled) Carnot stated that result in the guise of the general solution to the problem "given the virtual motion of any system of hard bodies, [. . .] find the real motion it will assume in the next instant"[29] and he symbolized the solution in the Equation (F) (see Eq. 2.2)

$$\int muU \cos z = 0.$$

which he had obtained through transforming an expression equivalent to conservation of live forces by means of the auxiliary idea of geometric motions. But in the *Principes fondamentaux de l'équilibre et du mouvement*, having elaborated the concept of geometric motions in a series of propositions, he stated the same thing in the form of a theorem:

> In the impact of hard bodies [. . .] the sum of the product of the quantities of motion lost by each of the bodies multiplied by its velocity after impact evaluated in the direction of the quantity of motion lost, is equal to zero.[30]

And he symbolized the relation in a mere corollary[31]

$$\sum MUV \cos \widehat{UV} = 0$$

The difference is indicative of that between the two books: in the *Principes fondamentaux de l'équilibre et du mouvement,* Lazare Carnot set out in a didactic order what in the *Essai sur les machines en général* he had established in a

[29] «*Problème* XX» (Carnot 1786, § XX, p 40; see also: *Ivi*, § X, p. 21).

[30] «THÉORÈME XI» (Carnot 1803a, § 168, p 139).

[31] For the Eq., see Carnot 1803a, § 169, p 143.

problematic one. A consequence was that he himself began the process of obscuring the distinctiveness of his own approach while improving its clarity. There was about the *Principes fondamentaux de l'équilibre et du mouvement*, moreover, not only an imitative quality in its relation to Lagrange but a regressive quality in its relation to theory. Carnot now tended to look back to the *Principle of Least Action* for the significance of his work concepts rather than forward to energy considerations. That he should have done so is natural enough for he never claimed to be an innovator, but to bring it out will require exemplification in a certain amount of detail.

Following the theorem just cited, a further proposition stated that in hard-body collision the sum of live forces prior to impact equals that following impact plus what the sum of the live forces would be if each of the bodies were moving freely with the velocity lost in impact:

$$\sum MW^2 = \sum MV^2 + \sum MU^2.$$ [32]

The point to notice here is that Carnot did not yet hold steadily in mind a concept of live force equivalent to kinetic energy. Sometimes what bodies were said to lose in collision was velocity or motion; sometimes it was live force itself, as in a corollary bringing out that the sum of live forces after collision was bound to be less than beforehand–but the case was restricted to hard body. He did have clearly in mind the convertibility of the quantities expressed in the dimensions of live force (kinetic energy) into those of moment–of–activity or latent live force (work or potential energy), but he was thinking of these matters dimensionally rather than conceptually. Only the concept of work did he have fully developed in all but name, and sometimes even in name. The role of energy he had to compound out of forces acting over distance (usually though not exclusively when it was a question of the measure of the force) or in time (usually though again not exclusively when it was a question of the operation of a machine process), and that necessity may explain why, the more he reflected, the more prominent the Principle of Least Action became in his thinking.

As he had done in the *Essai sur les machines en général*, Carnot turned to Least Action for the purpose of identifying the real motion among the infinity of geometric motions of a system of hard bodies. The motion that actually occurred would be that for which the sum of the products of the masses by the square of the velocity lost by each, was a minimum, or to put it more generally, such that

$$\delta \int MU^2 = 0.$$

Carnot considered that to be very beautiful and the fundamental law of hard–body collision. It might, he went on to show, be extended to cover elastic and even

[32]Carnot (1803a), §175, pp 145-146.

partially elastic bodies. When thus generalized it took the form,

$$\delta \int MUX = 0$$

(where X is the distance traversed by mass M with velocity U in time t), and this quantity, he pointed out, was that which Maupertuis had first denominated *Action*. Indeed, Carnot's view of his own accomplishment was that in the *Essai sur les machines en général* he had been the first to establish its applicability to the case of sudden or discontinuous change of motion in hard–body collision. Maupertuis had envisaged the matter so vaguely that he made no distinction between continuous and discontinuous change of motion. Euler had then distinguished the former case and made of it a rigorous, if still metaphysically justified, principle applicable to motion under central forces of attraction (Carnot 1803a, § 188, pp vi–ix, pp 163–164). Now in the *Principes fondamentaux de l'équilibre et du mouvement* Carnot thought himself to be exhibiting how superfluous metaphysical considerations were, and how generally the principle applied – only he did think preferable the form

$$\delta \int MU^2 = 0,$$

and that is the tantalizing aspect in later eyes since it does look very like the energy statement it could not yet be. He had not abstracted a notion of live force equivalent to energy from the properties of bodies: live force was lost completely in hard–body collision, partially lost in partially elastic collision; and Carnot was relying more fundamentally than ever on his workless geometric motions to bridge these differences. At the same time, he gave greater emphasis to the work principle at the expense of moment–of–momentum. In the *Essai sur les machines en général*, it will be re–called, conservation of the moment of the quantity–of–motion was the fundamental principle of mechanics sufficient for derivation of all laws of motion and equilibrium in all cases whatever.[33] Not so in the *Principes fondamentaux de l'équilibre et du mouvement*, where he invoked in its place a series of propositions about moments of rotation, moments of percussion, and d'Arcy's Principle of Areas,[34] and singled out instead as the fundamental conservative quantity the moment–of–activity of the general system with respect to any of its possible geometric motions, i.e., conservation of work (Carnot 1803a, § 197, p 176). But that appears to be something of an afterthought following the enthusiastic vindication of Least Action.

A final section headed "Considerations on the Application of Moving Forces to Machines" (Carnot 1803a, p 227) gathered together the remarks about machines

[33] See above Chapter 2, p 30.

[34] One wonders whether the papers of the chevalier Patrick d'Arcy had been an important source of inspiration to Lazare Carnot, but this is the only place he mentions them (Carnot 1803a, § 195, p 174). The relevant memoirs are: *Problème de dynamique* (d'Arcy 1752, II, pp 344–356) with an addendum (*Ivi*, pp 356–361), *Suite d'un mémoire de dynamique* (*Id.*, 1754, pp 107–108) and *Théorèmes de dynamique* (*Id.*, 1763, pp 1–8).

that Carnot had interspersed in the earlier edition to explain his reasonings, and combined them with the analysis of mechanical advantage and exhortations about the employment of power from its concluding scholium. The effect for the reader who knows only the *Principes fondamentaux de l'équilibre et du mouvement*, is to reduce to the level of an appendix what the reader of the earlier drafts can see to have been the motivation of the work as a whole. He had added nothing new to the earlier discussion about the factors of mechanical advantage or the way in which they might profitably be varied according to the economics of the process. He did here make one of his few references to the work of others, dissenting from Daniel Bernoulli's argument that a power source yielded much the same result whether the operator chose to augment the force or the speed. In the case of manpower and animal power, experiments by Coulomb had invalidated that hypothesis for the reason that it took no account of fatigue or boredom.[35]

A little further on Carnot also attributed to Daniel Bernoulli the maxim that in any proposed operation, the first thing to do is examine it in order to specify what effect intrinsically pertains to that operation, and avoid so far as possible producing any side effects. No work of Bernoulli was cited, but Carnot now said that it was pursuant to this principle that all shocks or sudden changes that are not essential to the construction of the machine are to be avoided, for shock always involves loss of live force and consumption to no purpose of a portion of the moment–of–activity developed. The only thing surprising here is that Carnot should thus have casually read back to Daniel Bernoulli the finding generally credited to himself, which remark illustrates further that what was involved in the development of engineering mechanics was less the knowledge of how things work than the articulation of the principles that all informed people knew more or less explicitly to be involved in processes.

In general, the argument about machine processes is briefer and easier to follow in the *Principes fondamentaux de l'équilibre et du mouvement* than in the *Essai sur les machines en général* or in the draft memoirs, and the concluding passage deserves quotation in order to exhibit further the gradual clarification in ideas that this edition reflects in what can be seen as the unwitting approach to energy considerations by way of the concept of work. After observing that the effect produced is always a live force, real or latent; comparable to the product PH of a weight P by a height H, or a force by a line, and that the moment–of–activity is, therefore, always the quantity to be economized for maximum effect whatever the process, whether it be a weight to be raised; a mill–wheel to be turned; a void to be created in the atmosphere, the sea, or some confined fluid; a machine to be started; or a system of bodies attracting each other in any proportion to the distances; and

[35]Carnot cited Coulomb's memoir on the employment of manpower, *Résultat de plusieurs expériences destinées a déterminer la quantité d'action que les hommes peuvent fournir par leur travail journalier suivant les différentes manières dont ils emploient leurs forces* (Coulomb 1799, II, pp 340–428). Coulomb first read this memoir to the Academy in 1778. See Gillmor (1971), chap. II. Carnot cited further a memoir of Euler, *de machinis in genere* (Euler 1751, III, p 254).

also whatever the motive agent, whether weights, wind, water, men, or animals in any combination at all, then:

> [. . .] whatever change may be occasioned in the system, the moment–of–activity consumed in a given time by the external powers always equals one–half the amount by which the sum of the live forces increases in the system to which they are applied during that same time, minus one–half. the amount by which that same sum of live forces would increase if each of the bodies moved freely on the curve it would describe, supposing it to be subjected at each point on the curve to the same force that actually does affect it: provided always that the motion changes gradually and that if spring–driven machines are involved, the springs at the end of the process are left in the initial state of tension.[36]

3.4 Comparing the Work of Sadi Carnot

Let us turn now to the memoir by Sadi Carnot, *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance*, published in 1824, the year after his father's death and 21 years after the *Principes fondamentaux de l'équilibre et du mouvement*. The similarity has often been remarked between Lazare Carnot's observations there on hydraulic machines and his son's model attributing motive force or power to the passage of heat considered as a real fluid falling from a higher to a lower level of temperature (Brunold, pp 37–40). In fact, however, when Sadi Carnot's memoir is read in direct succession to Lazare's writings, the son's inheritance appears fuller than the mere adoption of an hydraulic model for the flow of heat. Indeed, the *Réflexions sur la puissance motrice du feu* may be taken both for the foundation stone it certainly became in the science of thermodynamics and for the final item in a series of Carnot memoirs on the science of machines beginning with the prize essay of 1778. It is the latter relation that concerns us here.

There is no reason to think that Sadi Carnot would have objected to such an attribution. It is one that may be supported on biographical as well as substantive grounds. Graduated from Polytechnique in October 1814 (after having fought with many of his classmates in the brief and vain defense of Vincennes in March), Sadi Carnot completed his training with two further years of study at the school of military engineering in Metz. Until 1819 he led the garrison life of a second lieutenant. He then arranged to go on inactive duty in Paris in order to devote himself to study and technical research. All the while his father and younger brother were in exile in Magdeburg, where Sadi Carnot was able to visit them for a few weeks in 1821.

It was evidently after this visit that Sadi Carnot began concentrating his attention on the principles of heat engines, and first the steam engine. In addition to the published *Réflexions sur la puissance motrice du feu*, there is extant the manuscript of a brief "recherché" entitled "Investigation of a formula for the Motive Power

---

[36]Carnot (1803a), § 293, pp 261–262.

of Steam".[37] Scholars hold differing opinions about the date of this memoir. To the present writer it seems probable that it was composed somewhat prior to the *Réflexions sur la puissance motrice du feu*. It was in a genre of which there were many examples in the years around 1820, and which will be discussed briefly in the next chapter. Sadi Carnot obtained a value for the motive power of 1 kg of steam expressed as a function of temperature by employing Clément's law for the pressure of saturated vapors and Dalton's table relating vapor pressure to temperature. The discussion was clear and the derivation clever. In the interests of generality, he identified three stages in the operation of a steam engine: as the steam passes into the cylinder, it may be considered as expanding isothermally in the first phase and adiabatically in the second while the third stage was isothermal compression in the condenser. (These terms were not yet coined). But though recognizing elements of Sadi Carnot's analysis, the reader of the *Réflexions sur la puissance motrice du feu* will see this schematization as a partial one. The cycle was complete only with respect to the return of the piston to the starting point and not with respect to the temperature of the steam.

What appears to be a residue of that incompleteness marred the reasoning in the opening part of the argument of the *Réflexions sur la puissance motrice du feu* itself. It seems probable that the closer attention Sadi Carnot might reasonably be expected to have paid to his father's work on receiving in that same year the news of Lazare's death was what showed him the way to overcome that incompleteness and to put his argument on a fully general basis. We know from Hippolyte's memoir of his brother that when he, Hippolyte, returned to Paris after Lazare died in 1823, he found Sadi Carnot at work on the manuscript and was made to read and criticize important passages in point of their comprehensibility to general readers.[38] The brothers could scarcely have failed to talk then of their father's science.

The very word "Réflexions" in Sadi Carnot's title recalls the ruminative vein of Lazare's book on the calculus, while in analysis and subject matter the genre was that of his science of machines. Like the *Essai sur les machines en général*, the *Réflexions sur la puissance motrice du feu* was a treatise nonetheless rigorous for being verbally expressed and nonetheless general for being an adaptation of science to the principles underlying the employment of machinery. Despite the gratifying state (Sadi Carnot began) to which the development of steam engines had attained in practice, there existed no theory in the light of which their further improvement might be guided. Was there any limit to the power that ingenuity might draw from heat? The question was rhetorical and implied that there must be. What, then, were the optimum conditions for the design and operation of heat engines? Might not

[37] *Recherche d'une formule propre à représenter la puissance motrice de la vapeur d'eau*– the manuscript, *Un manuscrit inédit de Sadi Carnot,* was published by Gabbey and Herivel (pp 151–166). For a discussion of the question surrounding the date, see footnote 42 below.

[38] Hippolyte's biography is contained in a reprinted edition of the *Réflexions sur la puissance motrice du feu* published in Paris in 1878. References in the notes that follow are to the pagination of the facsimile of the first, 1824, edition published by Blanchard in 1953 (Carnot 1953).

other materials prove preferable to steam for developing the expansive force of vapor? Atmospheric air, for example–and this possibility, to which Sadi Carnot recurred often enough to confirm its appeal to his imagination, indicates that he had at some point found suggestive the model air engines of Niépce and Cagniard, of which Lazare Carnot had written enthusiastic accounts many years earlier.[39]

Those questions could not previously have been answered (to paraphrase further the *Réflexions sur la puissance motrice du feu*) for the reason that the availability of motive power in heat had never been considered in generality but only with respect to particular types of machines. In order that a fundamental theory might be found, it would be necessary to abstract from all particular mechanisms and from the properties of all particular materials and to consider how heat might produce motion as a problem independent of all contingencies. The theory must confine itself neither to steam engines nor even to vapor engines: it must embrace every conceivable heat engine and must model itself upon the theory of the classic machines. For there was a science that did indeed have the character of a complete theory (so held his father's son), resting as it now did upon the principles of mechanics itself. And only when the laws of physics should be sufficiently extended to embrace all the mechanical effects of heat acting upon bodies of any sort would the theory of heat engines be in a comparably satisfactory state (Carnot 1824, pp 6–9).

Sadi Carnot's memoir has been so fully commented, studied, and paraphrased in recent years that no need exists to summarize the entire contents or to enlarge on its later importance in the history of thermodynamics. For the present purpose it will suffice to identify those elements of the argument that derived from the work of Lazare Carnot. Sadi Carnot began by calling attention to a circumstance that always accompanied the production of motion by a steam engine. He chose to see it as a restoration of equilibrium in the caloric, by which he meant that heat is always transferred from a hotter to a colder body, from boiler to condenser. The process, he emphasized, involved the movement and not the consumption of caloric, and Sadi Carnot's analysis depended upon adopting the point of view that availability of motive power from heat presupposed some prior disruption of equilibrium in the distribution of caloric, and that reciprocally wherever a difference of temperature occurred, there existed the potentiality of drawing motive power from the transportation of caloric that would restore the state of equilibrium. In principle the agent might be anything, a metallic bar, for example, the reason being that in any object a change in temperature always involved a change in volume that might be harnessed. The choice depended on efficiency.

The question of how the motive power of heat might vary with the nature of the agent chosen to realize it, whether steam, air, metallic bars, or whatever sort of body, could be discussed decisively only for a given amount of heat and a given drop in temperature. Suppose that a body A was maintained at a temperature of 100° and a body B at a temperature of 0°. What would be the motive power (or work–the term itself was only five years from adoption) that could be delivered by transferring a

[39]See above Chapter 1.

given quantity of heat from A to B? Was it more efficient to employ one substance than another? The obvious advantage of vapors was that the temperature of gases would rise on compression and fall on expansion so that it was possible to disturb the equilibrium of the caloric in the same substance as often as desired. Moreover–and here Sadi Carnot gave the first hint of the analytical use he was about to make of the idea of a reversible process–it would always be possible to consider that steam might theoretically be employed in a manner the inverse of that of a steam engine for the purpose of disturbing the equilibrium in the caloric, i.e., for transporting heat from a colder to a hotter body by the expenditure of motive power.

In the initial outline of such a process, Sadi Carnot made it occur in the three stages of his unpublished *Recherche d'une formule propre à représenter la puissance motrice de la vapeur d'eau*: (1) In the first, the body A discharged the function of a boiler generating steam at its own temperature, i.e., isothermally. (2) In the second, the steam expanded (adiabatically) in the cylinder until its temperature fell to that of body B. (3) In the third, the steam was condensed at constant pressure by contact with body B, which, therefore, was filling the office of the cold water injected into a condenser, except that it remained at constant temperature and did not mix.

Having thus to a degree idealized and schematized the functioning of a steam engine, even as Lazare had done for ordinary machines; Sadi Carnot observed that these operations "[. . .] could have been done in one direction and also in the inverse order." (Carnot 1824, p 19). Steam could, first, be formed by employing the caloric of body B at its temperature. Second, compressed until it rose in temperature to that of body A. Third, condensed by further compression at the temperature of body A. In both directions, therefore, the first and third stages were isothermal and the intervening step adiabatic–though it is to be emphasized that these terms had not then been coined. The former sequence of operations produced motive power (or work) and transferred the caloric (or heat) from the higher temperature of body A to the lower of body B. The inverse operation expended motive power (or work) and returned the caloric (or heat) from body B to body A. But if the same quantity of vapor were involved and if no motive power nor caloric had been lost, then:

> [. . .] the quantity of motive power produced in the first case will be equal to that expended in the second, and the quantity of caloric transported in the first case from body A to body B will be equal to the quantity restored in the second from body B to body A, so that one could perform an indefinite number of similar operations without there finally being either motive power produced or caloric transported from one body to another.[40]

Evidently, then, Sadi Carnot was introducing the idea of a reversible process in the same place in his argument that Lazare Carnot had done in his, at the outset, and for the same reason: as an auxiliary in the reasoning to permit comparison of the initial and final states of a system by eliminating from consideration internal changes of work or energy. In the *Essai sur les machines en général*, it will be recalled,

[40]Carnot (1824), p 20.

Lazare's initial definition of geometric motion had been in terms of reversibility.[41] If (as seems probable) Sadi Carnot had the idea of the three–stage cycle set out in his *Recherche d'une formule propre à représenter la puissance motrice de la vapeur d'eau* manuscript before his father's death,[42] it is specially significant that he incorporated the idea of a reversible process only in the text of the *Réflexions sur la puissance motrice du feu* discussed with Hippolyte upon the latter's return from Magdeburg.

The next comparison is equally telling. Discussing ideal hydraulic machines in the *Principes fondamentaux de l'équilibre et du mouvement*, Lazare Carnot had brought out that it was a condition for *maximum* efficiency that there be no motion in the millstream that was not transmitted to the wheel, since any residual velocity in the water could in principle be harnessed on egress to produce an additional effect (Carnot 1803a, pp 248–249). Sadi Carnot for his part went on from the formulation of reversibility in ideal steam machines (for perhaps the literal translation of "machine à vapeur" helps bring out the carry–over of ideas) to argue that if there existed any method for employing heat more advantageous than the pair

[41] See above Chapter 2, pp 25–28.

[42] It is agreed that Sadi Carnot must have composed the manuscript of the *Recherche d'une formule propre à représenter la puissance motrice de la vapeur d'eau* at some time between November 1819 and March 1827. In publishing the text (Gabbey and Herivel) Drs. Gabbey and Rerivel concluded that a date prior to 1824, when the *Réflexions sur la puissance motrice du feu* appeared, was more probable than a later one. Mr. James Challey suggests that 1823 is the most probable date of composition because of Sadi Carnot's use of the *dyname* as the unit of motive power, Dupin having coined the word in a report to the Academy in April of that year (Challey 1971, *Dictionary of Scientific Biography*, III, p 83, n. 3). For reasons indicated in the argument of this section, I agree. Dr. Robert Fox disagrees, however. He kindly provided for me the proofs in his article *Watt's expansive principle in the work of Sadi Carnot and Nicolas Clément* (Fox 1970, 233–253). In that paper he explores the relations between Nicolas Clément (1779–1842) and Sadi Carnot very carefully. The main point is to establish the importance to Sadi Carnot's work of contemporary power technology. This it does admirably. Dr. Fox argues further that the manuscript *Recherche* represents an attempt by Sadi Carnot to compute a formula for the motive power of steam that would be applicable in actual engines as the highly abstract and unrealistic reasoning of the *Réflexions sur la puissance motrice du feu* could not be, specifically because in the manuscript memoir Carnot computed the motive power that would be developed in the phase of adiabatic expansion between any two temperatures instead of restricting it to the unreal case of a span of 1°. Dr. Fox may well be right. I doubt it, or doubt at least that this is the whole explanation because it reverses the configuration both of his work and his father's, which moved from the analysis of machine processes to theory rather than from theory to specific engineering application. Further, it seems more likely that the inclusion of the three–stage cycle applicable only to steam in the early passages of the *Réflexions sur la puissance motrice du feu* reflects rather the carry–over of an unperfected stage of his analysis into the final product, than it does that he should afterwards have reverted to this imperfect analysis in order to base an applicable calculation on it. There is a possibility that we are both right, and that the manuscript memoir, which does indeed have all the appearance of having been finished, represents the final development that Sadi Carnot, after publishing the *Réflexions sur la puissance motrice du feu,* then gave to the idea of a three–stage cycle with which he had begun his thinking. If so, it remains a problem that he did not publish it. Since almost all his manuscripts were burned after his death it is unlikely that the question will ever be decisively resolved.

of processes just described, it would follow that some greater quantity of motive power could be drawn from the flow of caloric in the first or forward process. It would then be possible to divert the excess or a portion of it to the job of driving the reverse process, that is to restoring the caloric from body B back up the temperature scale to body A. If that could be done, it would amount to an indefinite creation of motive force without consumption of caloric or indeed of any agent at all. It would amount, in short, to perpetual motion. Such a conclusion being contrary to "the laws of mechanics and sound physics", it was inadmissible. Excluding that impossibility afforded Sadi Carnot the basis for a preliminary statement of what he called the fundamental theorem:

> [. . .] the maximum motive power resulting from the use of vapor is also the maximum motive power that can be realized [from heat] by any means at all.[43]

A moment's reflection, consequently, would exhibit the condition for realizing the maximum motive power in general: it was

> [. . .] that there should not occur in the bodies employed to realize the motive power of heat any change of temperature that is not due to a change of volume.[44]

The reader familiar with later thermodynamic reasoning will immediately notice that the argument was not complete: it omitted the step of an adiabatic compression on the return process. The vapor in this initial illustration being steam, Sadi Carnot could not have incorporated such a stage since to vaporize water by compression without input of heat was physically unimaginable. He recognized the difficulty and, in moving on to a full and rigorous demonstration employing an air engine, admitted that in this preliminary sketch the vapor had not been supposed to be restored to its initial state (Carnot 1824, pp 36–37). Commenting on the discrepancy, T. S. Kuhn in his *Sadi Carnot and the Cagnard Engine* (Kuhn 1961, p 571, ft 9) has conjectured that Sadi Carnot began with a steam engine because of its greater familiarity to his prospective readers.

It is plausible or to think that these reasonings, and the conclusions so far drawn, reflect the direct carry over from Lazare's theory of machines into Sadi's early thinking. His having resorted to the argument from gradual change is persuasive, since reversibility plays the part of geometric motion. So also is the ensuing paragraph, in which Sadi Carnot explicitly invoked the analogy between the flow of heat and the flow of water. It was probably at this juncture that Sadi Carnot started to go beyond his father. He here began citing the extensive experimental demonstrations of his generation on the thermal aspects of the physics and chemistry of gases, matters on which he enlarged in the main body of his memoir. It was also precisely here that he put forward the rigorous demonstration of the "fundamental proposition" – i.e. that the utilization of vapor was the means of realizing the maximum motive power from heat – and did so in a fully general form. It was

[43]Carnot (1824), pp 21–22.

[44]Carnot (1824), pp 22–23.

on the basis of that demonstration, the full Carnot cycle, that he stated the theorem later regarded as his fundamental contribution:

> The motive power of heat is independent of the agents put to work to realize it; its quantity is determined uniquely by the temperatures of the bodies between which the caloric in the final result passes.[45]

We shall give the demonstration in Sadi Carnot's own words:

> Let us imagine an elastic fluid, air for example, shut up in a cylindrical vessel, *abcd*, provided with a movable diaphragm or piston, *cd*. Let there also be two bodies *A* and *B*, kept at a constant temperature, that of *A* being higher than that of *B*. Let us picture to ourselves the series of operations which are to be described[46] (Fig. 3.3):

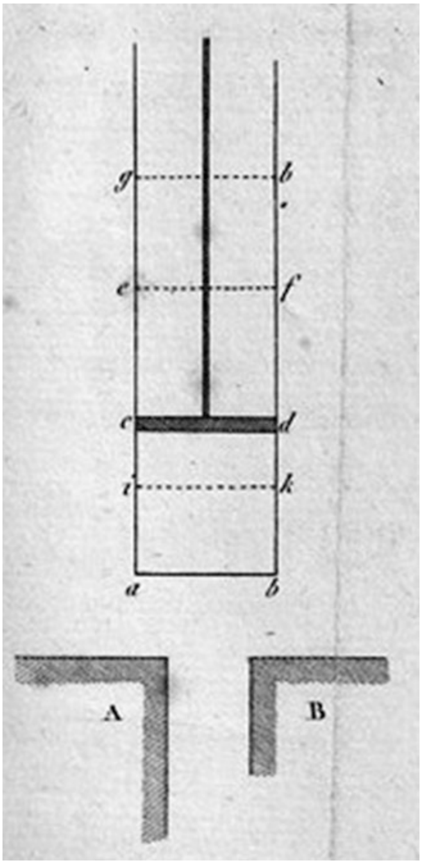

Fig. 3.3 One of Carnot's cylinders and operations in a cycle (Carnot 1824, p 118. With permission of the «Département de la reproduction, Bibliothèque Nationale de France, Paris»

[45]Carnot (1824), p 38.

[46]Carnot (1824), p 32.

1. Contact of the body *A* with the air enclosed in the space *abcd* or with the wall of this space – a wall that we will suppose to transmit the caloric (i.e. heat) readily. The air reaches, by virtue of such contact, the same temperature as the body *A*; *cd* is the actual position of the piston.
2. The piston gradually rises and takes the position *ef*. The body *A* is all the time in contact with the air, which is thus kept at a constant temperature during the rarefaction. The body *A* furnishes the caloric necessary to keep the temperature constant.
3. The body *A* is removed, and the air is no longer in contact with any body capable of furnishing it with caloric. The piston meanwhile continues to move, and passes from the position *ef* to the position *gh*. The air is rarefied without receiving caloric, and its temperature falls. Let us imagine that it falls thus till it becomes equal to that of the body *B*; at this instant the piston stops, remaining at the position *gh*.
4. The air is placed in contact with the body *B*; it is compressed by the return of the piston as it is moved from the position *gh* to the position *cd*. This air remains, however, at a constant temperature because of its contact with the body *B*, to which it yields its caloric.
5. The body *B* is removed, and the compression of the air is continued. Being isolated, its temperature rises. The compression is continued until the air acquires the temperature of the body *A*. The position of the piston passes during this time from the position *cd* to the position *ik*.
6. The air is again placed in contact with the body *A*. The piston returns from the position *ik* to the position *ef*; the temperature remains unchanged.
7. The step described under number (3) is repeated, then successively the steps (4), (5), (6), (3), (4), (5), (6), (3), (4), (5), and so on.

In these operations the piston is subject to an effort of greater or lesser magnitude exerted by the air in the cylinder. Its elastic force varies as much because of the changes in volume as of changes in temperature. But it must be noted that with equal volumes, the temperature is higher during movements of dilation than during those of compression.

> During the former, the elastic force of the air is found to be greater, and consequently the quantity of motive power produced by the movements of dilation is more considerable than that consumed to produce the movements of compression. Thus we should obtain an excess of motive power–an excess which we could employ for any purpose whatever. The air, then, has served as a heat engine; we have, in fact, employed it in the most advantageous manner possible, for no useless re–establishment of equilibrium has been effected in the caloric.[47]

The reader will notice that Sadi Carnot who like his father always uses ordinary language in his reasoning, nowhere employs the diagram that appears in many a physics text book to exhibit the Carnot cycle. His essay was largely ignored by physicists until the civil engineer, Clapeyron mathematicised it, *Mémoire sur la puissance motrice de la chaleur,* in 1834 (Clapeyron 1834, 153–190).

He there represented it graphically in a form that is too complicated to be worth explaining here and is included because it begins to look familiar (Fig. 3.4).

[47]Carnot 1824, pp 34–35. There is a translation by R. H. Thurston in Mendoza ed., *Reflections on the Motive Power of Fire and Other Papers on the Second Law of Thermodynamics by E. Clapeyron and R. Clausius* (Mendoza 1960).

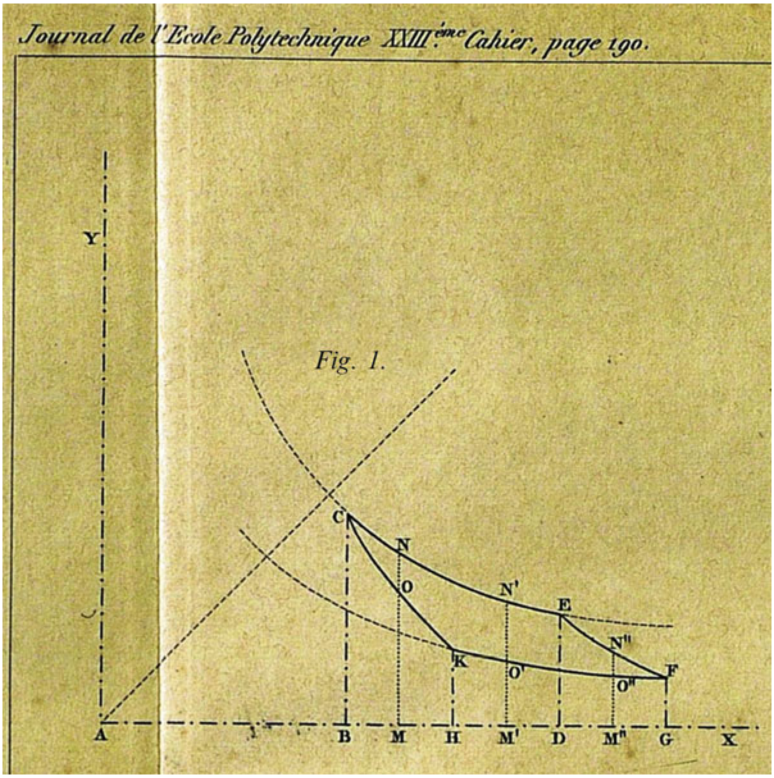

Fig. 3.4 Carnot's cycle in Clapeyron's diagram (Clapeyron 1834, Fig. 1, 190. «Collections École polytechnique Z 5 (1834)»)

That familiarity is complete in the graph given by Rudolf Clausius in his paper, *Über die bewegende Kraft der Wärme* (Clausius 1850, 368–397; 500–524) (Fig. 3.5).

Here the abscissa *oe* represents the volume and the ordinate *ea* the pressure on a unit weight of gas. The curve *ab* then represents the isothermal expansion, the

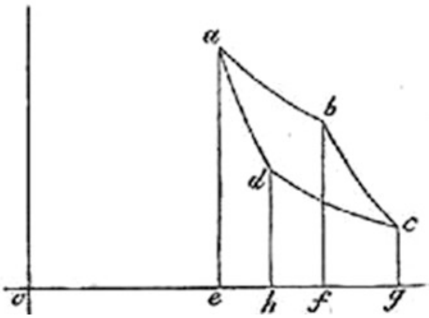

Fig. 3.5 Carnot's cycle in Clausius' diagram (Clausius 1850, 379)

curve *bc* the adiabatic expansion, the curve *cd* the isothermal compression, and the curve *da* the adiabatic compression. The figure *abcd* is an equilateral hyperbola and represents the work done by the gas in the course of the process. (The terms adiabatic and isothermal were not yet coined). Sadi Carnot, of course, was not thinking in terms of this four cornered graph, but rather of his piston and cylinder. Its first isothermal expansion is only part of what will later be Step 6. But once his cycle is established, his Steps 3, 4, 5, 6 constitute the cycle as normally described.

The above proof of the motive power of heat is as far as we need to follow Sadi Carnot in order to exhibit the full inheritance of the son from the father. No doubt the most significant items were the development of Lazare's geometric motions into Sadi's reversible process and their application to fully cyclic processes. Besides that, there was the exclusion of perpetual motion, axiomatic in Sadi Carnot, demonstrative and tutelary in Lazare Carnot; the generalization from principles of operation in particular types of machine to the principles of machines in general, applied to heat engines by Sadi Carnot; the restrictive mode of reasoning in which the maximum possibilities inherent in the operations were determined and then the conditions for realizing them defined; the curious combination of quantitative mode and verbal expression, such that the reasoning moves from the ideal case of changes occurring continuously and infinitesimally to the physical reality of discontinuous and irreversible changes of state in a system; the discussion of force in terms of what it can do, taken usually over distance when it was a question of its measure and over time when it was a question of its realization in mechanical processes. It cannot be said that either Carnot brought this tacit distinction to a decisive differentiation between proto–concepts of work and energy. Yet both of them assumed the conservation of the quantity measured by dimensions of work and energy, Lazare balancing his accounts between moment–of–activity produced and moment–of–activity consumed or live force, Sadi Carnot between motive power produced and caloric transported.

There was even the long wait for recognition, and the question whether what developed was the state of knowledge and interest in the subject that permitted the cogency and applicability of their theories to be recognized. There is this difference, however. One doesn't want to say that Lazare Carnot's work was shallow. But it blended into the physics of work and energy, which would have happened anyway. Sadi Carnot's work was deep in a way that his father's was not.

It founded the science of thermodynamics.